ON MR. HILL'S REVIEW OF THEORY OF MOON'S MOTION.

BY JOHN N. STOCKWELL.

In the March number of this Journal, Mr. G. W. Hill, of the Nautical Almanac Office, has given a somewhat extended "review" of the "Theory of the Moon's Motion, &c.," prepared and published by myself. Mr. Hill is a profound mathematician, and has rendered valuable service to astronomy in more than one department; his opinions, therefore, on subjects which he has thoroughly investigated, are entitled to great weight. Unfortunately, the "review" bears evidence of having been hastily prepared; for it is difficult to believe that a mathematical astronomer of Mr. Hill's attainments would have made statements so palpably untrue, had he taken the time to inform himself in regard to the subject in all its various bearings. It appears also to have been prepared in an unfriendly spirit; and it cannot properly be called an "impartial review", since the reviewer is content with the slightest allusion to its acknowledged merits, while he bears with tremendous power on what he fancies to be defects. This is to be regretted, because it shows the "reviewer" and the "reviewed" in an unfavorable light, and cannot fail to have less influence over the unprejudiced reader, than it would, had it been written in a spirit of friendly criticism. To correct for any false impression which the "review" may produce, has made it incumbent upon me to prepare the following reply; but no notice would have been taken of the "review" had it emanated from a less distingu'ed source.

On reading the very first paragraph of the "review" one is reminded of the celebrated answer of the caliph *Omar*, to the inquiry of *Amrou*, as to what disposition should be made of the Alexandrian library. "If the writings agree with the Koran", says *Omar*, "they are useless and need not be preserved; if they disagree, they are pernicious, and ought to be destroyed". In like manner, according to Mr. Hill, although the greater part of my results agree with what other calculators have found, and only a very few differ; those which agree are useless, and those that differ *must be wrong*; so that in either case, although obtained by a different method from what other calculators have employed, the putting of the results in a permanent form is to be regretted, and the labor bestowed on its preparation cannot be considered as much better than wasted. The motives which prompted the preparation of the work are said by the reviewer to be a "desire to controvert certain of the results arrived at by his predecessors; results, too, which have long been regarded by astronomers as definitely settled and acquired to seience".

Now, just how much has been "definitely settled and acquired to science" can be best determined by referring to the history of scientific development. And we need go back no farther than the time of Sir Isaac Newton for a starting point. Newton computed the mean motions of the node and perigee of the moon's orbit, and also several of the periodic inequalities in the motion of the moon; and the results obtained by him were "definitely settled and acquired to science", during a period of more than sixty years, and were confirmed by the most celebrated analysts about the middle of the last century. But notwithstanding these calculations of Newton, and their confirmations by his successors, Clairaut demonstrated that Newton's value for the motion of the perigee was not the true value due to gravitation, but was only about one-half of it. I am not, however, sufficiently acquainted with the periodical literature of that time to be able to state that the announcement, by Clairaut, that Newton had not correctly computed the motion of the perigee, created a flurry among his scientific brethren, and caused some of them to accuse him of seeking notoriety by controverting the results long since "acquired to science". On the other hand, I suspect they were profoundly grateful for his having so happily removed the greatest obstacle to the development of the lunar theory.

The next result of especial prominence to which I shall call attention, relates to the secular acceleration of the moon's mean motion in longitude. This was calculated by La Place as long ago as 1787, and confirmed by La Grange and afterwards by Damoiseau, Plana, Ponte´coulant and others who busied themselves with the subject; so that it was "definitely settled and acquired to science" during more than half a century. And yet, notwithstanding all these confirmations, Mr. Adams tells us that the result thus "acquired to science" is wholly wrong, and our reviewer accepts the statement without question.

It is unnecessary to multiply examples in which results "definitely acq'd to science" have been just as definitely abandoned, when the phenomena sought to be explained have become more perfectly known, and the *modus operandi* of the acting forces been brought into subjection to mathematical laws. Indeed the history of science is made up, in great measure, with the accounts of controversies about theories long since exploded; and the best names in science are often associated with theories that are no longer accepted. Thus, Newton and La Place both advocated the emission theory of light, and opposed the undulatory theory; but the emission theory, though once well established, has been pushed into the background, and at present is interesting only as a fiction of the past. These subjects are alluded to here, not by way of justification for any mistakes that I may have committed in the work under review, but to show the reader that mistakes have

been made by the most eminent men in every department of physical science; and I can recall no names of men who have attained to eminence in either theology, literature or science, who have not confessed to having made mistakes in the treatment of even simple questions; and I hope to show that my reviewer is no exception to the general rule.

I shall now pass from these general considerations to some of the particular cases mentioned by Mr. Hill. And first in regard to his statement that the value "$m = 0$, implies that we have either an infinitely short month or an infinitely long year; that is, the semi-axis major of the moon's orbit is infinitely small or the semi-axis major of the sun's orbit is infinitely great. Hence when we put $m = 0$, to be consistent, we are obliged to put $a \div a' = 0$". I cordially agree with Mr. Hill in this statement of general principles; and am profoundly grateful for his having stated the case so clearly. This is also valued the more highly from the fact, that, some years ago, he declined to discus the lunar theory at all, "because", says he," there is not a *single point of agreement* between us on which to base an argument".

And I will also venture to believe that there is one other point in connection with the lunar theory, on which we shall agree perfectly; and that is, that the solutions of the problem of the lunar perturbations, by La Place, Plana, Pontécoulant and Delaunay are entirely general in their nature, and are not restricted to the particular values of the elements of the sun and moon; except in the parts where the analytical values of the inequalities are reduced to numbers. If this be so, it is an easy matter to determine the perturbations the moon's motion would suffer if subjected to disturbing forces of different intensities. In fact the principal utility of a general solution of a problem consists in the facility with which the results may be applied to all problems of a similar character.

Now for greater facility in printing, we may suppose the sum of the masses of the moon and earth to be represented by unity, and then we may write equation (217) of the "Theory of the Moon's Motion", as follows,

$$m^2 = m' \frac{a^3}{a'^3}. \qquad [1]$$

In this equation a denotes the moon's mean distance from the earth, a', the sun's distance; m' denotes the sun's mass, and m the disturbing funct'n. Now we may vary m in three ways without changing the moon's distance First we may suppose that the sun's distance is changed while its mass remains the same; or second, we may suppose it to have a different mass while its distance remains unchanged; and finally we may suppose both m' and a' to vary, without altering the nature of the problem.

This being premised, let us take the equation corresponding to the third term of the second member of equation (B), page 72 tome II, of "*Théorie*

du Mouvement de la Lune, by Plana. By changing the notation of the argument so as to conform as much as possible to that used in my own work, it becomes

$$\frac{d^2\delta\mu}{dv^2}+(1-\tfrac{3}{2}\mu^2)\delta\mu+e\gamma^2(-\tfrac{21}{4}m^2+\tfrac{171}{128}m^3)\cos(v+\omega-2\Omega)=0. \qquad [2]$$

For the integral of this equation, Plana gives on page 76 the following

$$\delta\mu=e\gamma^2(-\tfrac{7}{8}+\tfrac{135}{64}m)\cos(v+\omega-2\Omega). \qquad [3]$$

This is wholly due to perturbation, because there is no equation depending on this argument in the elliptical value of μ.

Let us now inquire what would be the value of this equation, provided the sun's mean distance were *four* times its present distance. In this case the disturbing function would be $\frac{1}{64}$ of the actual value, and the year would be 8 times its present length. Equation [3] would become

$$\delta'\mu=e\gamma^2(-\tfrac{7}{8}+\tfrac{135}{512}m)\cos(v+\omega-2\Omega). \qquad [4]$$

Again, suppose the sun to be at *sixteen* times its present distance; the disturbing function would be $\frac{1}{4096}$ of its present value, and the year would be 64 times its present length; but for this case equation [3] would become

$$\delta''\mu=e\gamma^2(-\tfrac{7}{8}+\tfrac{135}{4096}m)\cos(v+\omega-2\Omega). \qquad [5]$$

In these two hypotheses the value of the disturbing function scarcely approaches the limiting value, zero; in fact the smaller value corresponds to the case of a planet and its satellite revolving around the sun nearly midway between the centre and outer limit of the solar system. It is therefore a legitimate hypothetical case, and will answer the purpose of comparison as well as though it were real.

If we now compare the values of $\delta\mu$, $\delta'\mu$ and $\delta''\mu$, omitting the term multiplied by m, which is equivalent to neglecting the square of the disturbing force, we find 1st, that the disturbance of the moon's distance from the earth would be the same as at present, if the disturbing force were only $\frac{1}{64}$th of its present value; and 2nd, that it would still be the same if the disturbing force were only the $\frac{1}{4096}$th of its present value. The most obvious conclusion to be drawn from these very curious results of analysis, is, that the disturbance is independent of the disturbing force, and would still remain the same were the disturbing force to vanish; in which case we shall have $m=0$; and the term depending on the given argument in equation [2] would vanish, and we should have $\delta\mu=0$. [6]

If we now take account of the terms depending on m in equations [3], [4] and [5], another very remarkable result follows; namely,

$$\delta\mu<\delta'\mu<\delta''\mu; \qquad [7]$$

that is, *the less the disturbing force the greater the disturbance!*

Were this very extraordinary result of analysis to be maintained by any body else than a profound mathematical astronomer, his sanity would at once be called in question.

According to Mr. Hill, this term expresses the transition from an orbit in motion to one at rest. If this be so, the transition becomes more violent in proportion as the disturbing force becomes less.

Mr. Hill next refers to my notice of Plana's calculation of the same inequality, by means of the variation of the elements; and becomes quite excited because I stated that a certain conclusion is not satisfactory. "Why a conclusion", says Mr. Hill, "legitimately deduced from correct principles, should be thrown aside at a mere *arbitrium*, certainly surpasses our powers of explanation." Now without reflecting at all on Mr. Hill's "powers of explanation", it is at least charitable to suppose that he is acquainted with the principle of the *reductio ad absurdum*, so much employed in geometry.

If a conclusion supposed to be legitimately deduced from correct principles, leads to absurd or impossible results, it is fair to believe either that the *principles* are *not* correct, or else that the *conclusion* is *not legitimately* deduced. Take a case in point: La Place deduces certain conclusions respecting the tides, which conclusions are confirmed by Mr. Airy, by means of a somewhat different method of investigation; but these conclusions were not satisfactory to Mr. Ferrel, because they involved the absurd consequence that *very high tides* would be produced even when the dist'bing force vanished. But it often happens that conclusions may be legitimately deduced from correct principles, and yet not be applicable to the problem which gave rise to them. Mr. Hill certainly knows, that in the computation of the orbit of a planet, the equation by which the distance of the planet from the sun is determined is of the eighth degree, and consequently may be satisfied by eight different values of the unknown quantity. But only one of them is applicable to the particular planet whose orbit is sought to be determined.

How, then, are we to discriminate between so many values, and fix upon the correct one? Or are there really eight different positions of the planet at the same time, in order that Mr. Hill's powers of explanation may not be overtaxed? Surely not. In order, then, to discriminate between the different values of the unknown quantity and fix upon the correct one, we test them, and find which ones lead to absurd or impossible conditions in regard to the place of the planet. For example, one value might give a *negative* radius vector to the planet, another might give a *negative distance* from the earth, and these must of course be rejected; still another might bring the planet even *into the eye of the observer*, which would of course be the wrong place for a planet. And by continuing in this way till we find one value which will satisfy both *physical* and *analytical* conditions we ac-

cept it as the correct value of the unknown quantity, and do not hesitate to reject the seven values which merely satisfy the analytical conditions.

Returning now to the particular case under consideration, we find that Plana has given the two following expressions for the differential variations of the eccentricity and perigee of the moon's orbit. See tome I, p. 97.

$$\frac{de}{dv} = \tfrac{21}{8}m^2e\gamma^2\sin 2(\omega-\Omega), \qquad \frac{d\omega}{dv} = \tfrac{21}{8}m^2\gamma^2\cos 2(\omega-\Omega). \qquad [8]$$

These are strictly secular equations, since they depend wholly on the variations of the elements of the moon's orbit, and are independent of the posi'n of the disturbing body. Now Plana has integrated them as if they were what are called periodic equations; simply because the perigee and node complete a revolution in a comparatively short period of time. It is found that the quantity $2(\omega-\Omega)$ varies by the quantity $3m^2v$. Now if we put the variable part of the angle $2(\omega-\Omega)$ equal to $3m^2v$, and call the integrals of equations [8], δe and $\delta\omega$ we shall have,

$$\delta e = -\tfrac{7}{8}e\gamma^2\cos 2(\omega-\Omega), \qquad \delta\omega = \tfrac{7}{8}\gamma^2\sin 2(\omega-\Omega). \qquad [9]$$

The principal term in the value of μ is

$$\mu = e\cos(v-\omega); \qquad [10]$$

and the variation of μ arising from any finite variations of e and ω, will be given by the equation

$$\delta\mu = \left(\frac{d\mu}{de}\right)\delta e + \left(\frac{d\mu}{d\omega}\right)\delta\omega = \cos(v-\omega)\delta e + \sin(v-\omega)e\delta\omega. \qquad [11]$$

If we substitute the preceding values of δe and $\delta\omega$, in this equation, it will become

$$\delta\mu = -\tfrac{7}{8}e\gamma^2\cos(v+\omega-2\Omega); \qquad [12]$$

which is the same as the first term of equation [3].

In the case of the moon disturbed by the sun, the argument of δe and $\delta\omega$, requires three years to complete a revolution. Were the sun placed at *four times* his present distance, the period of the argument would be 192 years; but the values of δe and $\delta\omega$ would remain the same, since the time *increases* in the same ratio as the force diminishes. Again, were the sun *sixteen times* his present distance, the period of the argument would be more than 12000 years, and yet the variations of δe and $\delta\omega$ would remain unchanged, since the diminution of the force would still be compensated by the increase of the time. But whatever be the amount of these variations of δe and $\delta\omega$, their substitution in equation [11] always gives the same value of $\delta\mu$.

If, then, the value of $\delta\mu$ remains the same whether the perigee and node move much or little, it is obvious that it is independent of this change of the elements, and would still subsist were the elements constant, in which case we should have $\delta e = 0$, $\delta\omega = 0$, and then equation [11] would give $\delta\mu = 0$, the same as before found.

It is easy to trace these curious results to the values of the integral given by equation [9]. If we suppose that at a particular epoch, the eccentricity and longitude of the perigee are denôted by e and ω, it is evident that at that epoch we should have $\delta e = 0$, $\delta\omega = 0$; but equations [9] show this to be impossible, because when δe vanishes $\delta\omega$ is a maximum, and *vice versa.*

The maximum value of δe in equation [9] amounts to 80″, and has a period of three years, or 40 revolutions of the moon. Now in general, the variations of the elements, in orbits of small eccentricity, are much greater than the variations of the coordinates; but according to Plana's calculations we have here a *monthly* equation amounting to 114″ growing out of a small secular equation whose period is 40 months. Were we to apply the same principle to the perturbations of the earth by Venus, we should find the following values $\delta e' = 449''\cos(\omega''-\omega')$, $e'\delta\omega' = 449''\sin(\omega''-\omega')$, ω'' denoting the longitude of the perihelion of Venus; and these quantities would give for the perturbation of the earth's longitude

$$\delta v' = 898''\sin(n't-\omega'').$$

That is to say, a small secular inequality, in the elements of the earth's orbit having a period of 90000 years, gives rise to an *annual* equation of 898″ in the earth's longitude. Now there is no such inequality in the earth's longitude, and consequently any method of computation which gives such an inequality must be erroneous. But if the principle is not applicable to the motion of the earth, it is not applicable to the motion of the moon; and the results derived from its application must be erroneous.

There is another equation of the moon's longitude, of considerable importance in the lunar theory, and which has for its argument $nt - \omega'$, or the moon's distance from the sun's perigee. This arises from the motion of the moon's perigee, and like the one we have been considering, it increases in magnitude as the disturbing force diminishes;—the distance of the disturbing body being supposed to remain unchanged.

There is also an important equation of the moon's latitude, which is produced by the secular variations of the node and inclination of the moon's orbit, the argument of which is $nt-2\omega+\Omega$. It is also subject to the same peculiarities as those already mentioned; and it is unnecessary to enter more into the details of the question.

Our reviewer next insinuates that the terms depending on the square of the disturbing force annoy me very much. But in this he is mistaken. These terms are simply the perturbations arising from the previous perturbations produced by the first power of the disturbing force; and in order to correctly compute them, the terms depending on the first power must be correctly computed. But the terms arising from the first power may be accurately computed without any reference to those arising from the second and higher powers of the disturbing force.

Mr. Hill next states that the largeness of the error which my results would imply as existing in the lunar tables ought to have led me to suspect the legitimacy of my own conclusions. I cheerfully accept this statement of my reviewer; and will only reply to it as Prof. Adams replied to a similar objection from his opponents in the controversy about the moon's secular acceleration; namely, that it is purely a question of theory, with the decision of which observation has nothing whatever to do.

But perhaps nothing can better illustrate the supreme efforts Mr. Hill has made to become acquainted with the work he has attempted to review, than his remarks about the inequality depending on the angular distance between the perigees of the sun and moon. After admitting that my equation [24] is simpler than any which have before been applied to the computation of such inequalities, he says that he cannot find any proof of these equations; and charges me with having adopted them quite arbitrarily.

Now I admit that the demonstration of these equations has not required a separate chapter, as is usual in most works on the lunar theory; and this will probably account for his not being able to find it; but the demonstration is, nevertheless, in the work under review. And as to his statement that equations [23] are inconsistent with each other, I will only say in explanation, that that part of the work was designed more for the non-techn'al reader, and that I did not wish to complicate it with abstruse mathematical formulæ. But I now perceive that it was an oversight, as it has had the effect of misleading so good a mathematician as Mr. Hill. In the body of the work, however, the formulas are all right. And as to his statement that the formula gives an infinitely great coefficient to the inequality, when the disturbing force is infinitely small, it is sufficient to say that the formula meets all the requirements of the case. If the disturbing mass is infinitely small, the value of h will be infinitely small, and the inequality would be small; but if a is small, the period of the inequality would be long, and the mere novice in science would be able to understand that a very small force acting during a very long time is sufficient to accomplish very considerable work. In fact, were the perigees of the sun and moon to remain stationary, the moon would be acted upon by a constant tangential force (unless their longitudes were the same or diametrically opposite); and it is evident that the resulting inequality would ultimately become infinite.

Mr. Hill next says that the secular equation depending on the oblateness of the earth, and arising from the diminution of the obliquity of the ecliptic to the equator does not exist, because the inclination of the equator to the fixed ecliptic of any given date varies very slowly and proportionally to the square of the time. Now the fact that the inclination of the moon's orbit to the apparent ecliptic remains constant, was one of La Place's happiest

discoveries; and so long as the apparent ecliptic approaches the equator, the the moon's orbit must approach it also. The fixed ecliptic has, therefore, no more to do with the problem than has the plane of Jupiter's orbit.

Lastly, in regard to the secular inequality, I would say that I have never before attempted a thorough investigation of that subject. It is true, however, that some fifteen years ago I published a pamphlet in which I attempted to show that the new terms found by Mr. Adams, had no existence; and as yet I have seen no reason to change the views there expressed in regard to that matter.

In general, we may say that small secular equations of the elements of both, planets and moon, are produced by the large periodic inequalities to which these bodies are subjected; but for a large periodic inequality to be produced from a small secular inequality, is inconsistent with both reason and correct calculation. And from whatever point of view we approach the subject, it becomes more and more apparent that our lunar tables in use at present are based on very defective theories; and the only wonder is, that they can be made to represent the moon's motion as well as they do. I can, therefore, as yet, see no reason for recalling or modifying my statement that our present lunar tables are really erroneous by some of the smaller terms of the *third order*, instead of being correct to terms of the *seventh order* as has heretofore been supposed.

[*Correct'n.*—In the foregoing paper, for μ read u, except in first parentheses of line 4, p. 85.]

NOTE BY THE EDITOR.—At the time Prof. Wood's article on Limits (see p. 80) was put in type, we had not seen Newcomb's Algebra to which reference is there made. As it seemed improbable that Professor Newcomb would pursue the line of argument there attributed to him, we have since procured a copy of the Algebra alluded to and find that Prof. Wood has (unintentionally no doubt) misrepresented Prof. Newcomb's argument. As stated by Prof. Newcomb, the argument is entirely legitimate and the conclusion is *unquestionably correct.* The argument, as stated by Prof. Newcomb, is as follows:—

"Suppose AB to be a line of given length. Let us go one-half the dist. from A to B at one step, one-fourth at the second, one-eighth at the third, etc. It is evident that, at each step, we go half the distance which remains. Hence the two principles just cited apply to this case. That is,

"1. We can never reach B by a series of such steps, because we shall always have a distance equal to the last step left.

"2. But we can come as near as we please, because every step carries us over half the remaining distance."